

*(Slide 1 – Title page of original Phys Rev Letter)*

## Observation of Antiprotons

Herbert Steiner – October 28, 2005

### Introductory Comments

You might be wondering why I am here talking about the discovery of the antiproton. The answer is quite simple. It is by default. I was lucky enough to be involved in this experiment while I was a graduate student here, and unfortunately the other members of the experiment are no longer available to tell the tale. Fortunately, Owen Chamberlain is here with us today, and although he isn't speaking I would like to acknowledge the pivotal role he has played not only in the antiproton experiment, but in the many other endeavors we have jointly undertaken over the years.

What I want to do then is to go back to those heady days, and tell you just a little of what I remember about those times. My leaky memory has been refreshed and augmented by Owen's oral history, Emilio Segrè's autobiography, Owen's and Emilio's Nobel Lectures, John Heilbron's history of the Rad Lab, by the original Phys Rev article, by rummaging through the LBNL archives and by discussions with colleagues and friends.

*(Slide 2 – Outline of Talk)*

*(Slide 3 – Segre, Wiegand, Lofgren, Chamberlain, Ypsilantis)*

### The Cast of Characters

*(Slide 4 – Emilio Segre)*

- *Emilio Segrè (50): He was born in Tivoli (near Rome) in 1905. He was the first student of Fermi at the University of Rome. He made important contributions in atomic spectroscopy, slow neutrons and was the first to discover artificially produced elements (Tc, As). He was a group leader at Los Alamos, Professor of Physics, UC Berkeley and the leader of our group at the Rad Lab. He had a great nose for what is important in physics, and an uncanny ability to go to the heart of a problem. He didn't suffer fools lightly, so it was a good idea to be well prepared before talking with him. He had the political and scientific muscle, as well as the intellectual stature to insure that his group got its share of resources and beam time. At the time of the antiproton experiment he was no longer a hands-on experimentalist in the sense of building or running equipment, but he was very effective as an intellectual and administrative resource. He died in 1989.*

*(Slide 5 – Clyde Wiegand)*

- *Clyde Wiegand (40): Born in Oakland, CA in 1915. Graduated from Williamette College in Oregon. Worked as an engineer/announcer in a radio station in Fresno, before entering UCB as a graduate student in 1940. He was a student of Segre. In his autobiography A Mind Always in Motion Segre writes:*

“One day I was trying to build a power supply for an electronic apparatus on my own. A student started looking at me, and after a while, with a half-disgusted expression, asked whether he could help me. I was happy to accept, and within half an hour he provided me with a much better supply than I could ever have made. The student was Clyde Wiegand, and this started a collaboration and friendship that lasted for the rest of our careers.”

Wiegand went to Los Alamos with Segrè. He completed his PhD in 1950. He was an unusually gifted experimentalist, especially with respect to electronics. He was extremely well organized and meticulous in his approach to physics. He had a real knack for making equipment and experiments work. He developed one of the first distributed amplifiers, and continued over the years to come up with innovative physics and technical ideas. In my opinion he and Owen Chamberlain were the prime movers in this experiment. He was very effective in turning his ideas (and those of others) into practical devices. He was very quiet and self-effacing with a wonderful sense of humor. He was our technical guru and the person who made the experiment work. Died in 1996.

*(Slide 6 – Owen Chamberlain)*

- Owen Chamberlain (35): Born in San Francisco in 1920. Graduated from Dartmouth College in 1940. Entered UCB as a graduate student in 1940. Again I quote from Segrè's book:

“In one of the optics courses there was a student who amused himself by finding flaws in the lectures. His objections, always polite, were often well taken and showed a critical and alert mind. I appreciated the young man, who obviously was interested in the course and used his head, and I made friends with him. He was Owen Chamberlain ....”

During WWII Owen went to Los Alamos with Segrè. After the war he resumed his graduate physics education at U. Chicago under Fermi. He did his PhD thesis on neutron scattering from liquids in 1949. He was appointed Instructor in Physics at UCB in 1949 (with PhD not yet completed). He worked with Segrè and students on nucleon-nucleon scattering experiments at the 184" synchrocyclotron. He had a unique ability to explain almost any concept in physics in his own inimitable style. He was the one the students would go to whenever they wanted to understand something they were baffled by. He was (and is) bright, energetic, deeply committed to physics. He and Clyde Wiegand were the persons most responsible for the detailed planning of the experiment. At the time of the pbar experiment he was an associate professor.

*(Slide 7 – Tom Ypsilantis)*

- Tom Ypsilantis (27): Born in Salt Lake City, Utah. Graduated in Chemistry from The University of Utah in 1949. Came to Berkeley for graduate study in physics under Segrè. He made seminal contributions to experiments with polarized protons at the 184" synchrocyclotron. His PhD thesis (1955) was on polarized proton-proton scattering. He was one of the most imaginative

physicists of his generation. Always an optimist he, more than any of his contemporaries, had an innate ability to think “outside the box”. He was a stimulating and talented colleague, who would go on to make other important contributions to particle physics. He was actively involved in the setting up and running of the experiment. He died in 2000.

### Origins

As you just heard from Ed Lofgren the energy of the Bevatron was chosen so as to make it kinematically possible to produce antiprotons, and consequently the search for antiprotons at the Bevatron became a high profile physics objective for physicists both inside and outside the Rad Lab. You will hear about some of the work of others from Gerson Goldhaber and Bill Wenzel, but the genesis of the effort in our group probably dates back to the fall of 1953 when Owen Chamberlain returned to Berkeley after having spent the summer at Brookhaven. In his oral history he tells of conversations with colleagues at BNL that set him to thinking about how to do this experiment. He firmly believed that antiprotons would be found, and he was therefore particularly stimulated when he heard of a bet between two of his BNL colleagues, Hartland Snyder and Maurice Goldhaber. Let me tell the story in Owen’s own words:

“ I heard that there had been a bet between Hartland Snyder and Maurice Goldhaber, with Maurice Goldhaber betting some large sum – it could have been \$500; it seemed like a huge sum at the time. Maurice had bet that the antiproton didn’t exist and Hartland Snyder had said it did.

Well, I have great respect for Maurice Goldhaber as a physicist, and I suspect he made the bet when he was a little drunk, but even when drunk, Maurice Goldhaber is a good physicist. So if someone of the stature of Maurice thought maybe antiprotons didn’t exist, then this was a real spur to showing that they did. And I think it was at that moment that I decided, “By Jove, this is what I want to do”.”

I asked our next speaker, Gerson Goldhaber, who is Maurice’s brother, what he knew about this bet, and he confirmed that, indeed, it was made, and that after the antiproton discovery was announced Maurice actually wrote a check, which Hartland put away but did not cash.

By the way a little later, I think it was shortly before we actually started taking data, there was another bet. This time it was between Ed McMillan (anti-antiproton) and Emilio Segrè (pro-antiproton). The amount was 25 cents. I suspect Segrè had no hesitation in cashing it.

But back to the story of the antiproton experiment. In his oral history Owen goes on to say:

“Now in the antiproton business, I think I took an idea which was laying around for everybody to fuss with. I ran with it, of course with Clyde Wiegand, because the two of us worked very intently on it.”

During much of 1954 Owen and Clyde were busy with p-p triple scattering experiments at the 184”-synchrocyclotron, but somehow they found the time,

often in the late afternoon and evening, to design and prepare the antiproton experiment. There was an uncharacteristic air of secrecy to all this, because they were of course not alone in their quest for being the first to detect antiprotons. Several groups including those of Wilson Powell, Chaim Richman, Burton Moyer, and above all Ed Lofgren were hot on the trail.

### The Method

The two main choices were (1) Determination of mass and charge from measurements of momentum and velocity or (2) Detection of proton-antiproton annihilation. Owen and Clyde decided to measure mass and charge. The basic idea was to take negative particles produced at  $0^\circ$  in a pop-up copper target located inside the Bevatron, bend them into an external magnetic channel using magnetic field of the Bevatron, and then measure the velocities of all the particles in this momentum-analyzed beam. Two independent methods were used to determine the velocities of these particles -- time-of-flight and Cherenkov counters. The amount of material in the beam was kept to a minimum.

Before continuing with the description of this method let me make a few comments about the relevant kinematics. (*Slide 8*). The nominal maximum energy of the protons in the Bevatron was 6.2 GeV. This is just slightly above the threshold energy of 5.6 GeV needed to produce antiprotons from free protons. Note that at threshold the antiprotons go forward and have a kinetic energy of  $mc^2 = 0.938$  GeV ( $p = 1.625$  GeV/c,  $\beta = 0.866$ ). To produce the slightly lower energy antiprotons that we actually used in our experiment (or to produce them at larger angles) requires more energy, or the use of a nuclear target in which the target nucleons are moving.

I think the beauty of the experiment lies in its simplicity. A schematic diagram of the apparatus is shown in *Slide 9*. A double focusing spectrometer, based on a suggestion by Oreste Piccioni, was used to transport the beam of particles along its predetermined path. Two scintillation counters, S1 and S2, separated by a distance of 40 feet were used to measure the time of flight, and two Cherenkov counters, C1 and C2, were used to discriminate between the antiproton and the pions in the beam. C1 was used to veto the pions and kaons, whereas C2 was tuned to select particles having  $\beta = 0.77 \pm 0.015$ . In order to increase the time separation of the pions and antiprotons, as well as to keep the velocity of the antiprotons below the threshold of the veto counter C1, the momentum of the beam was chosen to be 1.19 GeV/c ( $\beta = 0.78$ ). At this momentum the slower moving antiprotons required 12 ns more than the pions to travel the 40 foot distance between S1 and S2. A photograph of the all-important second leg is shown in *Slide 10*, which also shows some of the participants.

The velocity-selecting-Cherenkov counter (VSC), also known as the pickle barrel and/or secret weapon, was conceived by Owen and built by Owen and Clyde. I brought along the original prototype model that was tested at the 184" synchrocyclotron. The final version is schematically shown on *slide 11*, and a picture of the components is shown in *slide 12*. The response of this counter as a function of velocity is shown in *slide 13*.

There is an interesting footnote to the VSC story. In his oral history Owen recalls that Segrè had gone to the East to a conference where he heard a description of a Cherenkov counter from Sam Lindenbaum. Segrè was under the impression that it was a threshold counter, but upon his return to Berkeley the picture he drew of it on the blackboard suggested to Owen that it was really a velocity-selecting counter that would count only in a band of speeds. So he and Clyde made a test model, which is the little round thing up here. They satisfied themselves that the thing would work, and then tested it at the cyclotron. On the basis of that test they built the larger size that was used at the Bevatron.

Owen says:

“Well, now this turned out to be exactly the same thing that Lindenbaum was doing; the principle was the same, the geometry was the same. A minor difference was that we used a piece of glass for the radiator, and he had used a sample of gas under high pressure. But the principle of the counter was the same. And I learned, I think before we did the antiproton experiment that this was really Lindenbaum’s counter. I seem to remember I talked to Lindenbaum about it and he said, ‘Well, very good. Good Luck’.”

The time-of-flight system required the use of fast electronics so that the 12ns time difference between the pions and the antiprotons could be fully exploited. Thanks to Clyde, special low loss Styrofoam insulated 50 ohm coax lines were laid from the location of the counters to the counting room which was several hundred feet away. The nanosecond had not yet entered the standard vocabulary, instead we talked of milli-microseconds, and in fact we often still used a unit of time called the shake with 1 shake = 10 milli-microseconds. Clyde was our electronics expert and he saw to it that we had the circuits that would do the job. Despite his expertise, or perhaps because of it, he insisted on displaying the pulses from all counters on a fast 4-beam oscilloscope, and then making a photographic record of them.

Clyde designed the 4" diameter quadrupoles. Remember, this was in 1954/55. There were no computers and no CAD systems. He did all the calculations, made all the technical drawings, and specified all the parameters. I think the magnets were actually built by Westinghouse. That company had a slogan: “You can be sure if it’s Westinghouse” and they lived up to it. You could be sure that there would be water leaks. You could be sure that there would be short circuits. You could be sure that if there was any way they could screw up, they would. Despite all that we were able to get these quadrupoles to work just fine. Owen, with help from Tom and me, measured the focusing properties using wire orbits.

### The Experiment

In going through the quarterly “Bevatron Operation and Development” reports from 1954/55 I could find no mention of antiproton-related activity until the 3<sup>rd</sup> quarter of 1955. John Heilbron, in his book *Lawrence and his Laboratory* comments that between November 1954 and September 1955:

"Most of the experiments concerned proton scattering, pion production and scattering, and the life history of the K meson."... "The pursuit of kaons, though exciting and rewarding, had an air of déjà vu: Once again a great accelerator made possible the detailed study of particles found in cosmic rays. Another quest beckoned, the detection of a particle of fundamental importance then not yet found among nature's products. Several groups began to look for antiprotons early in 1955. (HS comment: It was really in July 1955) . Two hoped for quick victories using detectors that had worked well for mesons. One, under Chaim Richman, stuck emulsions in a beam of negative particles from a metal target. Another, under Wilson Powell, used a cloud chamber. They both hoped to find evidence of the end of the career of antiprotons in annihilation explosions. They found nothing."

In *Slides 14 and 15* I show excerpts from the 3<sup>rd</sup> quarter report, and in it we see the first indication that negative proton searches were on the operating schedule. Heilbron writes:

"Time at the Bevatron did not come for the asking. The Laboratory physics division set priorities for the big machine to which its users conformed in negotiating schedules under Lofgren's diplomatic management. Various contingencies affected the implementation of the proposed schedule; the machine might not work, the appointed group might not be prepared, the preceding experiment might be prolonged. Log sheets from the earliest days of physics research on the Bevatron show both the ideal and the real worlds, the scheduled experiments and those performed. During the first week of August Segrè's group was scheduled for three of the six days of Bevatron operation, and ran for five; during the second and third weeks it had no time, while Lofgren's and Powell's groups sought antiprotons in their own ways; on August 29 Segrè's group returned and ran as scheduled until the Bevatron broke down on September 5. On the 21<sup>st</sup>, a week after operating crews had revived the machine, Lofgren's group was to begin a four day hunt for the antiproton. Instead it ceded its time to Segrè's group, which immediately got its first antiproton counts. For the next month the entire research effort at the Bevatron went into confirming and extending the counts. The physics division decreed that Segrè's equipment would remain in place indefinitely; and money was found to increase the nominal operating hours from 65.5 to 81 a week."

The experiments operated 24 hours a day, and when our group did run we usually had Clyde on shift during the day, while Tom Ypsilantis and I would be there at night. Owen, who had teaching duties, was there in the afternoons and evenings, and Segrè looked in whenever he could. Graduate students Tommy Elioff and Don Keller also helped. Even Ernest Lawrence often dropped by in the evening. In order to stem the flow of inquisitive visitors into the counting room Clyde installed a blackboard just outside the entrance on which he



summarized the status of the experiment and the score of the World Series. This is shown in *Slide 16*. Among the important tasks that often fell to the junior members of the group was the developing and scanning of the hundreds of feet of film that we used to record the pulses from the counters, and the refilling of collapsed helium bags in the secondary beam channel. Initially we had slightly mistuned the secondary beam magnets, but that was quickly corrected, and on September 22<sup>nd</sup> the first antiproton candidate was seen.

## Results

The pion to antiproton ratio in our beam was typically about 40,000:1. Clyde who was our electronics expert, and understood our detection system from top to bottom, insisted that we photographically record all pulses from all counters. Typical examples are shown in the three traces reproduced in *Slide 17* that show the pulses produced by a pion, an antiproton and an accidental coincidence between two particles. Using this method we soon convinced ourselves that we could cleanly separate the antiprotons from the pions. *Slide 18* shows a histogram of the flight time for (a) pions, (b) antiprotons, and (c) accidentals. By reversing the polarity of the magnets we could transport protons down our beam channel and check the performance of the system. *Slide 19* shows the measured mass of these protons (solid curve) beam and that of the antiprotons (points). The measured excitation function for producing antiprotons relative to pions is shown in *slide 20*. As expected the production of antiprotons decreases rapidly as the proton beam energy is decreased.

By the middle of October Clyde, Owen, Tom and I were firmly convinced that we had unambiguous evidence for the existence of antiprotons, and that the results should be published quickly. After all the other groups were breathing down our necks. The next step was to convince Segrè that we should go ahead. To turn Segrè's critical insights to our advantage Clyde devised a strategy of reverse psychology in which we would appear to be just a little hesitant, and let Segrè convince us to forge ahead. Clyde's plan worked to perfection, and on October 19<sup>th</sup> we submitted our paper based on 39 events to Physical Review, Letters to the Editor. It was published on October 26<sup>th</sup>.

## Aftermath

Later on October 19<sup>th</sup> a press conference was held at the Rad Lab at which the announcement of the discovery was made. The headline in that afternoon's Berkeley Daily Gazette proclaimed "Grim new Find at UC". Apparently the reporter covering the story had heard that if antiprotons were to collide with him or his newspaper they would blow up.

Shortly after the discovery Segrè informed the Vatican. Perhaps he wanted to make sure that he wouldn't end up like Galileo. Fortunately for him there were no papal repercussions.

For several years after the paper was published we received repeated letter from an outraged reader, who lambasted us for suppressing a far greater discovery.

He noted that the histogram in *Slide 21* showed the pion peak at about 38 ns and the antiproton peak at 51 ns. The distance between the counters was stated to be 40 feet (i.e., corresponding to a time of 40 ns for a particle traveling with the velocity of light), so clearly we were either too stupid or too devious to report that we had observed tachyons.

In 1959 Chamberlain and Segrè were awarded the Nobel Prize for this discovery. The blackboard reporting the announcement and the subsequent press conference are shown in *Slides 22 and 23*.

A series of follow-up experiments was undertaken at Berkeley over the next few months and years. These included annihilation studies in emulsions, experiments with antineutrons, bubble chamber exposures with separated beams, and cross section measurements. It soon became obvious, however, that the energy of the Bevatron was too low to allow it to compete effectively in the antiproton arena. Only nine months after the antiproton discovery Ed Lofgren announced that the Bevatron was “obsolete in design and in a few years will not even be in the class of high energy physics”. The work would continue at other accelerators.

For the benefit of the younger generation let me end with some of the lessons I learned:

- (1) Be in the right place at the right time
- (2) Be lucky
- (3) Have good collaborators
- (4) Have a lot of talented and dedicated people helping you
- (5) Don't mess with the Piccionis of this world

\*\*\*\*\*

I received the following message from Maurice Goldhaber on November 1, 2005:

Postscript to the Berkley Symposium on 50 Years Since  
The Anti-Proton Discovery.

Maurice Goldhaber, BNL

Since I was unable to attend the Symposium, I learned only later that my (in-)famous bet with Hartland Snyder was discussed there. Since, according to Dirac's theory, an anti-proton should exist, the energy of the bevatron was chosen to be sufficient for producing anti-protons. I should like to explain my thoughts before the discovery of the anti-proton.

In the early fifties Hartland had invited my wife and me to a party. As soon as we arrived he shook my hand, in his usual tempestuous way, and said “I bet you \$500 that the anti-proton exists,” and with no chance of explaining why I felt that the anti-proton needs to be confirmed experimentally, I accepted the bet. My wife immediately said “this is foolish,” but I was too proud to withdraw



my handshake.

Though Hartland may have been aware then or a little later that I had been puzzling over the paradox that our world is built only of protons in spite of the fact that anti-protons were expected to exist, a paradox later considered to be resolved when theoretical developments showed that one kind of particle would prevail. Before that I even went so far as to consider the existence of an anti-world, but did not publish this idea until after the discovery of the anti-proton, ["Speculations on Cosmology," *Science*, 124 218 (1956).] The Russian theorist, Markov, called me crazy, but nowadays cosmologists speak of "multi-verses" and I was told recently that mine was the first step in this direction.

Hartland was an honorable man and perhaps thought, because of my reservations, that he had trapped me. He did not cash the check; after his death his widow, hard up financially while caring for three children, cashed it. So it did at least some good!

Luis Alvarez thought that the existence of my bet implied that the discovery of the anti-proton deserves the Nobel Prize!

A few years after the discovery of the anti-proton it became clear that the properties of protons and anti-protons are not completely described by Dirac's original theory, since they interact differently with leptons of different helicity, perhaps a *weak* justification for my bet!

# Observation of Antiprotons\*

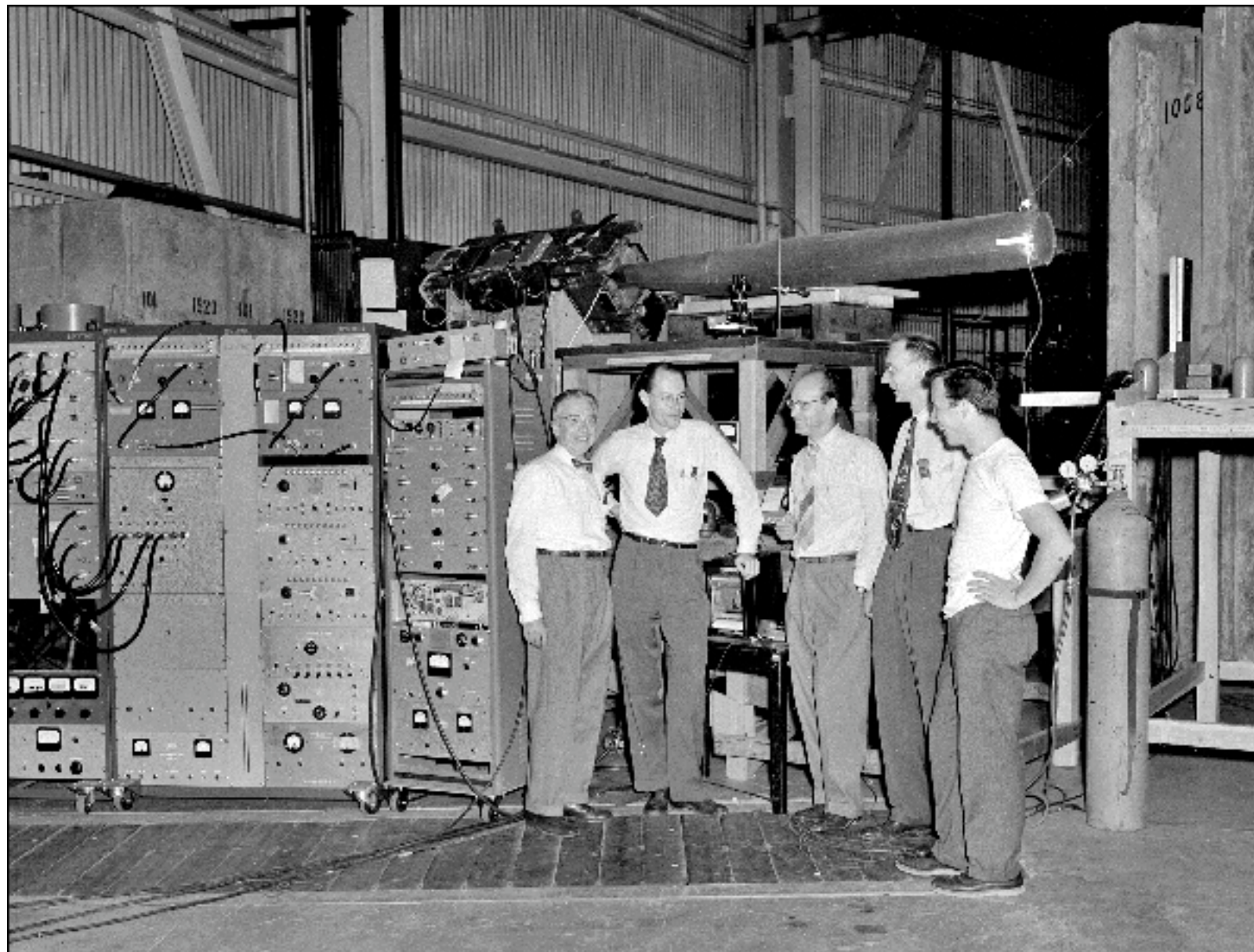
OWEN CHAMBERLAIN, EMILIO SEGRÈ, CLYDE WIEGAND,  
AND THOMAS YPSILANTIS

*Radiation Laboratory, Department of Physics, University of  
California, Berkeley, California*

(Received October 24, 1955)

# Outline

- Cast of Characters
- Origins
- Method
- Experiment
- Results
- Aftermath

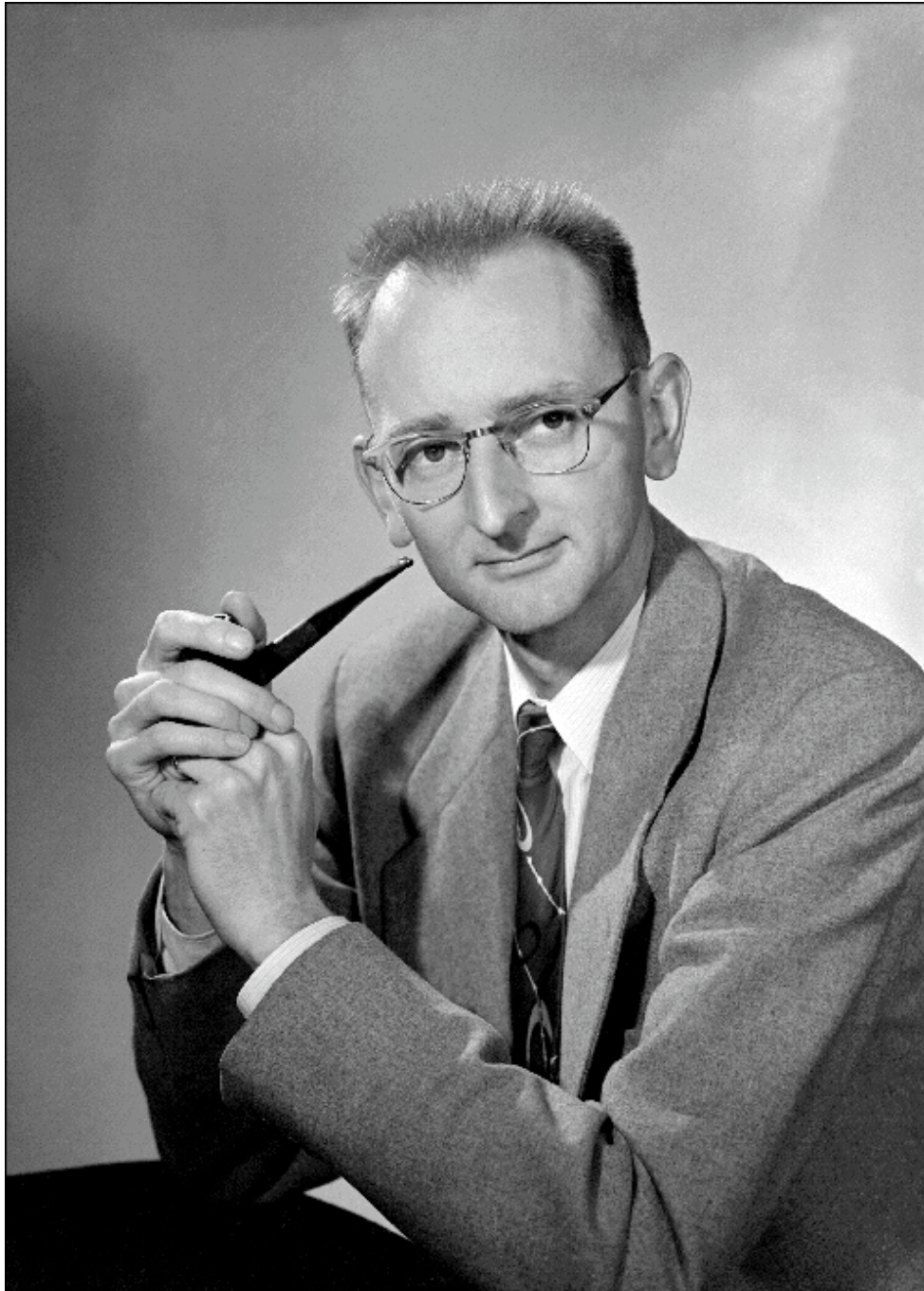






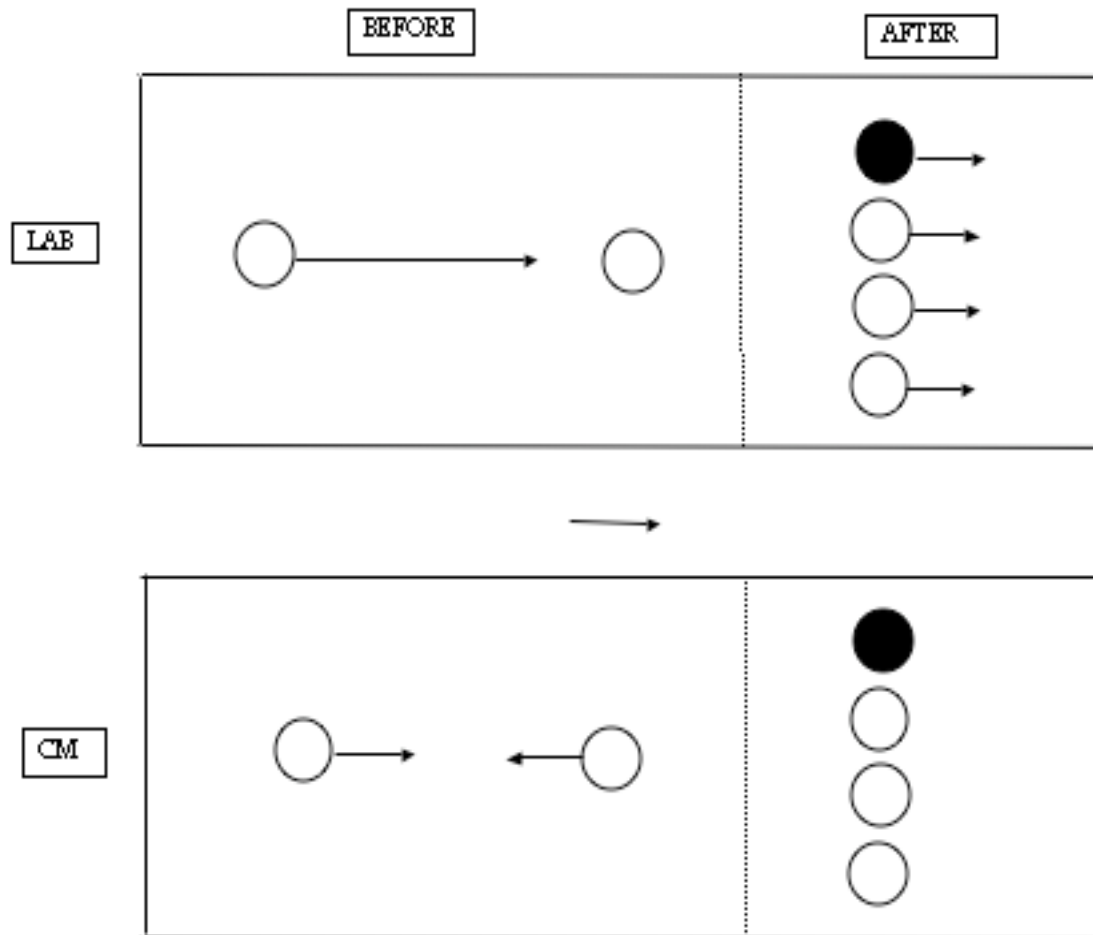








## THRESHOLD KINEMATICS



Assumes stationary target nucleon

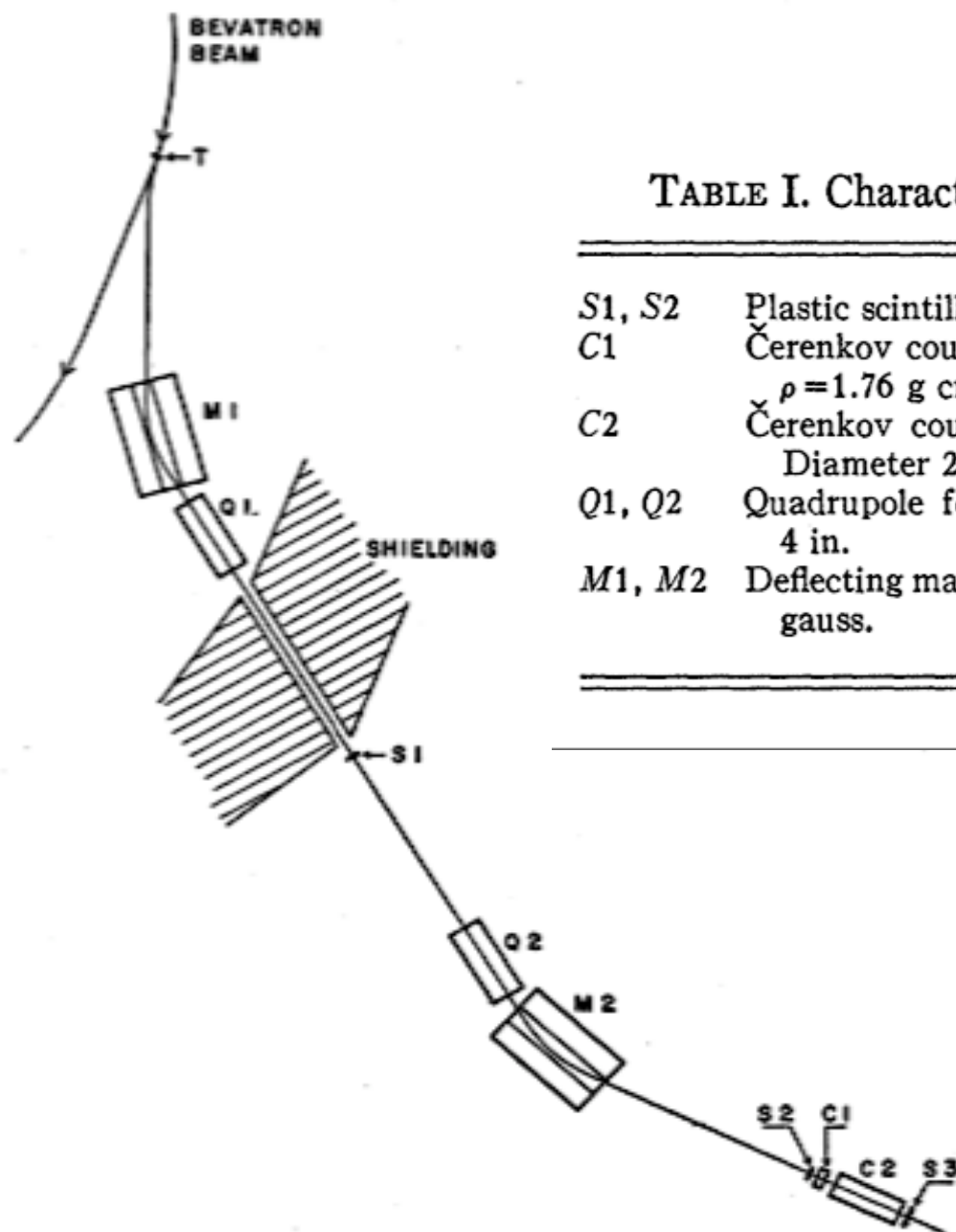


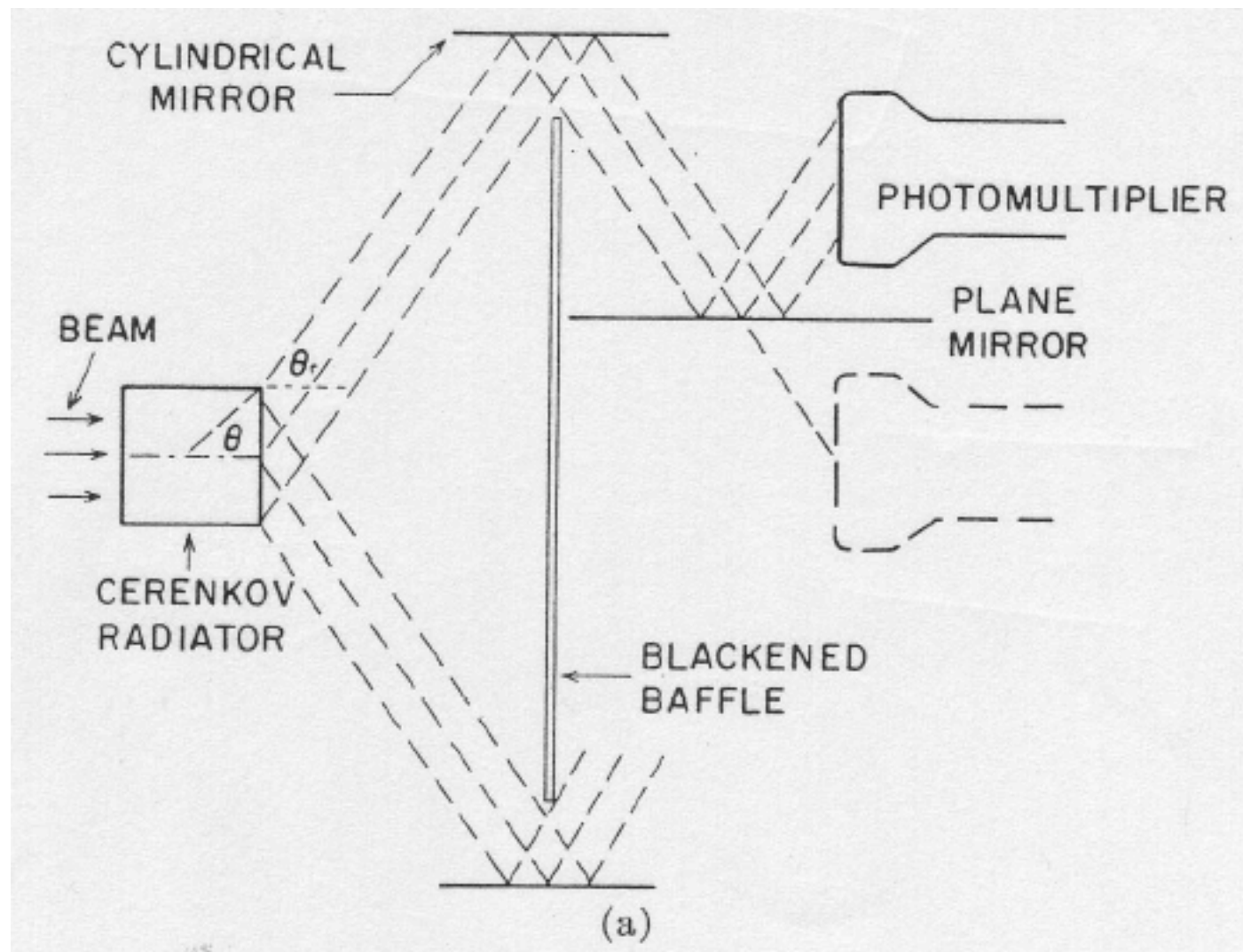
TABLE I. Characteristics of components of the apparatus.

S1, S2	Plastic scintillator counters 2.25 in. diameter by 0.62 in. thick.
C1	Čerenkov counter of fluorochemical 0-75, ( $C_8F_{16}O$ ); $\mu_D = 1.276$ ; $\rho = 1.76 \text{ g cm}^{-3}$ . Diameter 3 in.; thickness 2 in.
C2	Čerenkov counter of fused quartz: $\mu_D = 1.458$ ; $\rho = 2.2 \text{ g cm}^{-3}$ . Diameter 2.38 in.; length 2.5 in.
Q1, Q2	Quadrupole focusing magnets: Focal length 119 in.; aperture 4 in.
M1, M2	Deflecting magnets 60 in. long. Aperture 12 in. by 4 in. $B \cong 13\,700$ gauss.

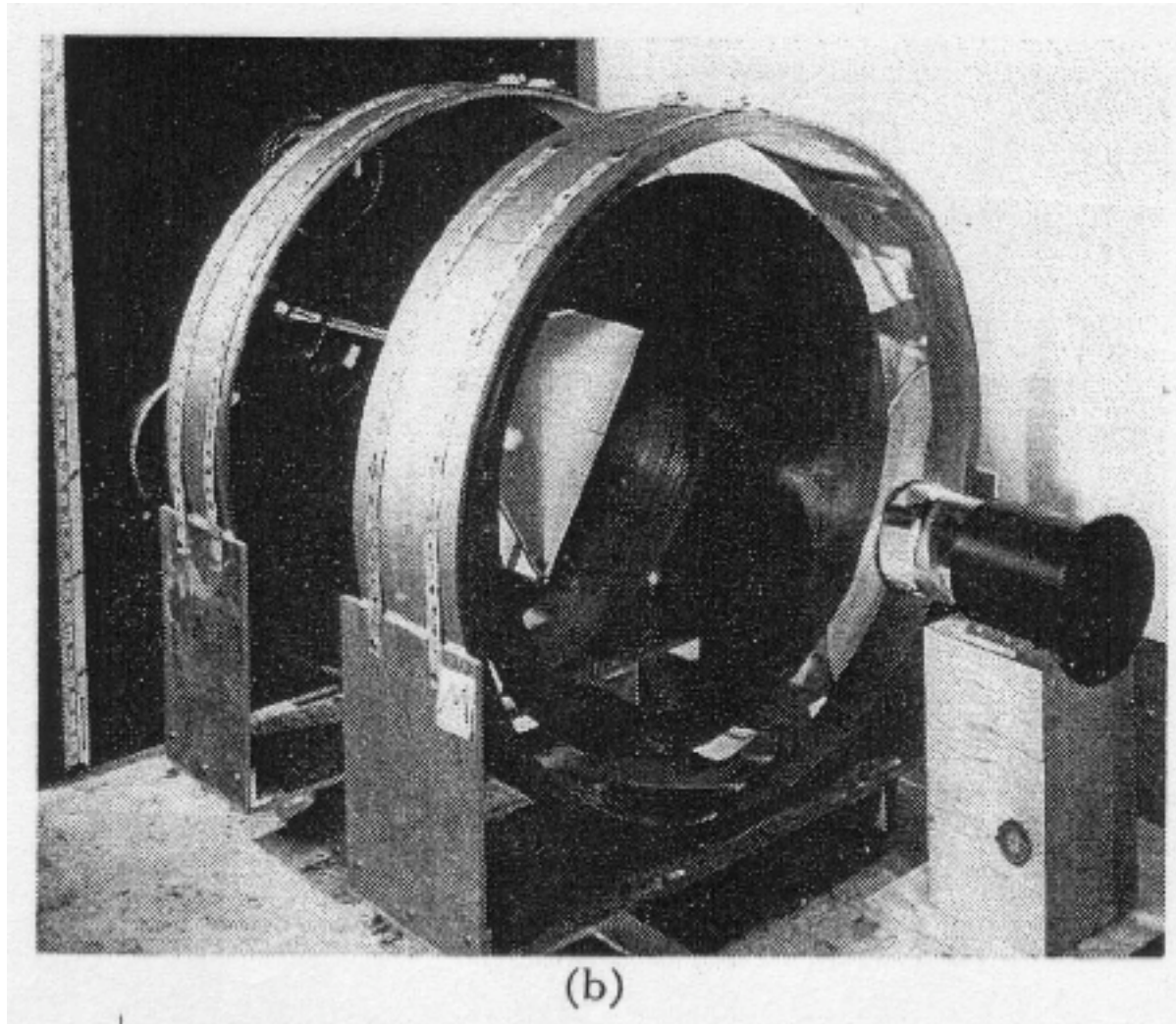
FIG. 1. Diagram of experimental arrangement.  
For details see Table I.

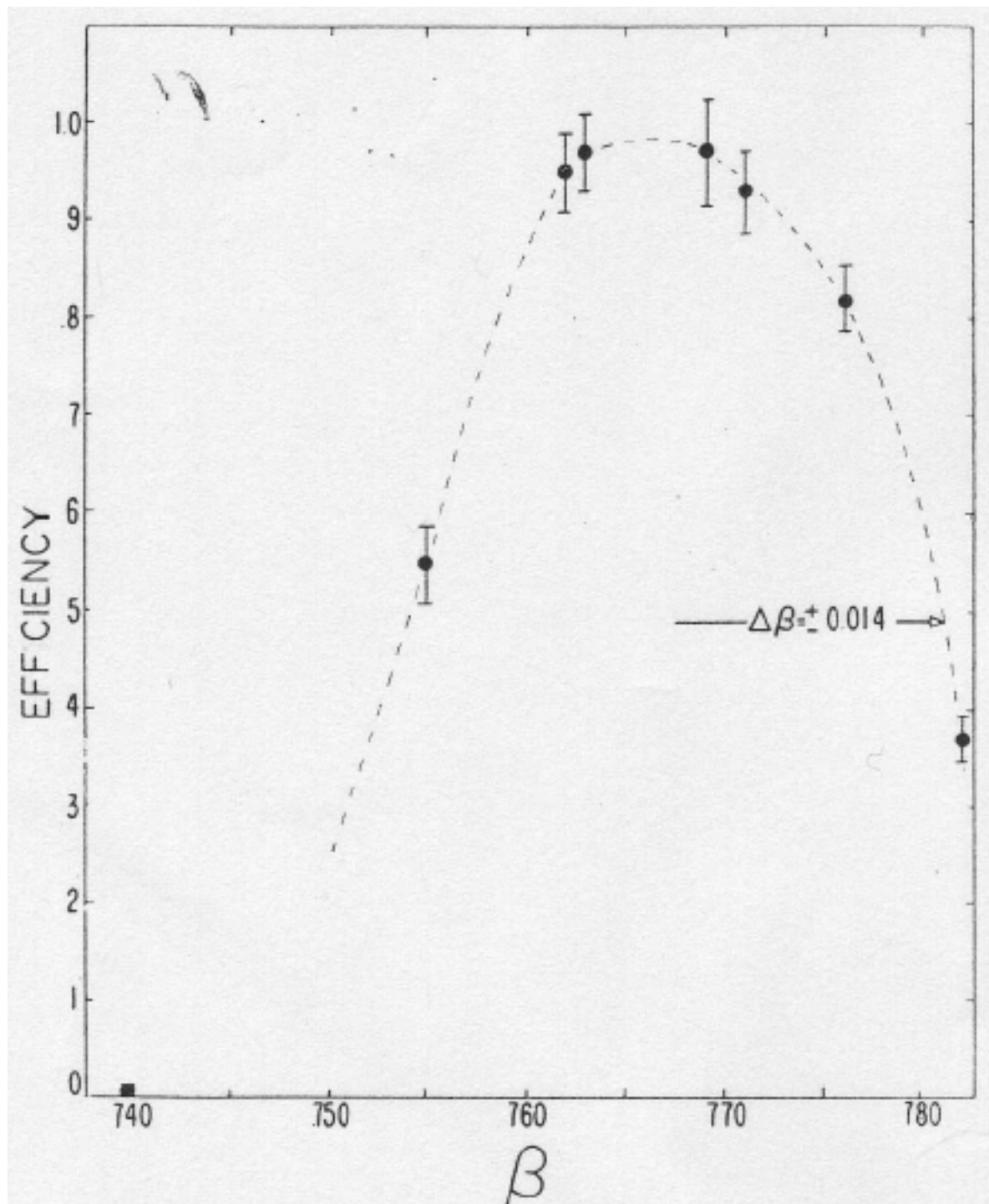












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# Radiation Laboratory

BEVATRON OPERATION AND DEVELOPMENT, VII

August, September, October 1955

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Table V

Bevatron Experimental Research Program  
August, September, October 1955

## INTERNAL GROUPS

Group	Experimenters	Experiments
BARKAS		
	Barrett	Emulsion exposures, neutron spectrum, and neutron interactions.
	Cole	Assessment of new Eastman Kodak nuclear emulsions.
LOFGREN		
	Cork, Horwitz, Murray, Wenzel	$P^-$ detection, using a multi-Cerenkov counter telescope.
	Cork, Wenzel	P-P scattering, using counters; 2 Bev, 4 Bev.
	Cork, Horwitz, Murray, Wenzel	$P^-$ annihilation investigation, using counters.
	Heard	Beam resonance, n-value study.
MOYER		
	Bostick, Cence, Wikner	Study of deflected particle flux between Bevatron magnet slabs.
	Osher, Parker	Decay products of mesons and hyperons, using a $\gamma$ -ray spectrometer.
	Osher, Parker	Excitation function for associated production of $\pi^0$ mesons.
	Brabant, Wallace	$\gamma$ -ray spectrum from $\pi^0$ -meson decay, using a $\gamma$ -ray spectrometer.
	Bostick, Kaplan, Wikner	$\pi^-$ total cross section, using counters (2.8 Bev/c). Emulsions in $\pi^-$ beam (2.8 Bev/c).
	Wikner	$\pi^+$ total cross section, using counters.
POWELL		
	Fowler, Lander	$P^-$ search, using a cloud chamber.
	Maenchen, Wright	P-P scattering of 5.5-Bev protons in 35-atmos diffusion cloud chamber.

## INTERNAL GROUPS

Group	Experimenters	Experiments
SEGRE		
	Chamberlain, G. Goldhaber	Antiproton search, emulsion exposure.
	Chamberlain, Steiner, Ypsilantis, Wiegand	$P^-$ detection and counting; mass measurement and excitation function (1.19 Bev/c).
WINSBERG		
	Benioff	Polyethylene target bombardment.
	Shudde	U, Al, Pt foil bombardment.
	Winsberg	Al, Mn foil bombardment.
VAN ATTA		
	Ise, Pyle	$\pi^-$ total cross-section measurement in Cu and C, using a cloud chamber.
LIVERMORE NUCLEAR FILM GROUP		
	Violet, White	Emulsion exposures, neutron spectrum, and neutron interactions.
EXTERNAL GROUPS		
Experimenters	Institution	Experiments
CÜER		
	University of Strasbourg Strasbourg, France	Internal emulsion exposures at proton beam energies of 2 Bev, 4 Bev, 6.2 Bev.
LORD		
	University of Washington	Internal emulsion exposures at 6.2 Bev. Emulsions in $\pi^-$ beam (2.8 Bev/c).
TICHO		
	UCLA	Emulsion exposures, neutron spectrum, and neutron interactions.



## PROGRESS OF ANTI PROTON EXPERIMENT

YANKEE

15

NOTE: ALL RESULTS ARE PROVISIONAL & SUBJECT TO RECALL, MEET WITHIN THE FAMILY

DETECTED: 38 negative particles, mass  $940 \pm 70$  MeV ( $1840 \pm 140$  MeV) [6.1 to 6.3 Rn]

0

John S. G. 1971

2

2050<sub>ms</sub>

All reduced energy (4.3 kG) set for mass 1740, found 3 in a time 15 would occur at full energy (4.1 kG) required mass 1740.  
 PRESENT OPERATIONS Set for mass 1740. Beam energy 4.1 kG.

PRESENT OPERATIONS set for March 1940. Beam energy 4.1 to 4.4 Bev

A most probable threshold is 5.1 Bw with lower limit at 4.4 Bw for 1 stage process.

nummer my. förändr. för. m. m.

38

1

number comparison

1,810,000

48005

Momentum of ray particle from: 1.187 Bev.

$C = \frac{\text{total particles}}{\text{vol. of sample}}$  of each particle r/f p mass: 572

Energy

572 MeV

4:30 PM

Oct. 6

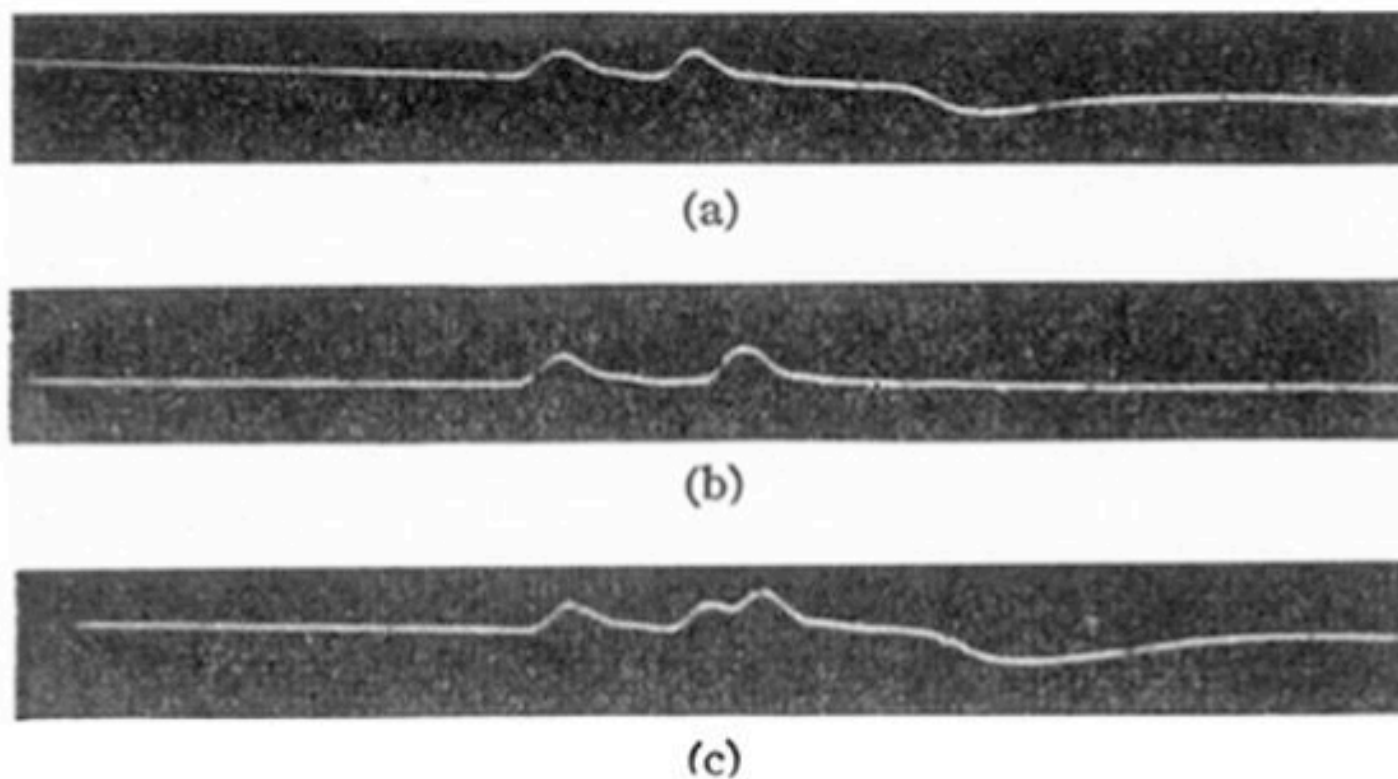


FIG. 2. Oscilloscope traces showing from left to right pulses from  $S1$ ,  $S2$ , and  $C1$ . (a) meson, (b) antiproton, (c) accidental event.



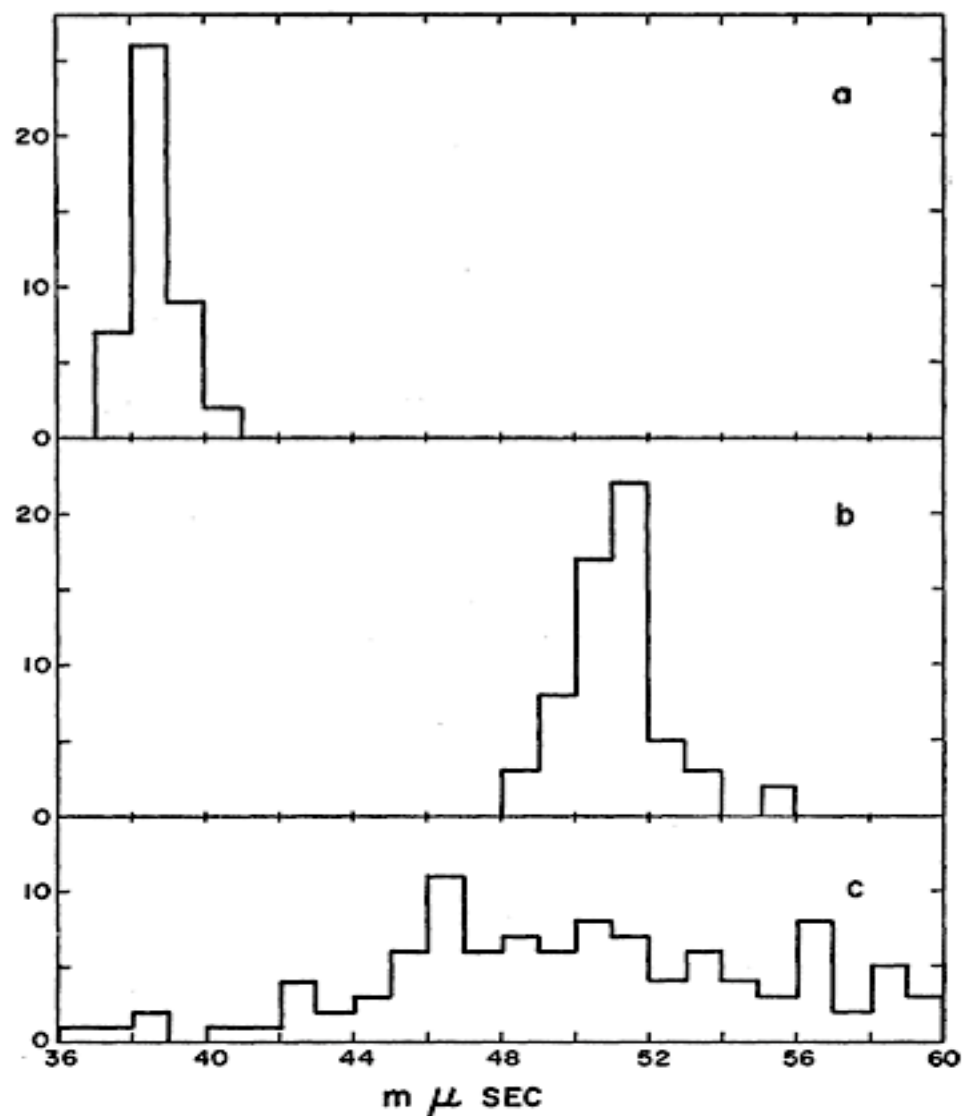


FIG. 3. (a) Histogram of meson flight times used for calibration. (b) Histogram of antiproton flight times. (c) Apparent flight times of a representative group of accidental coincidences. Times of flight are in units of  $10^{-9}$  sec. The ordinates show the number of events in each  $10^{-10}$ -sec intervals.

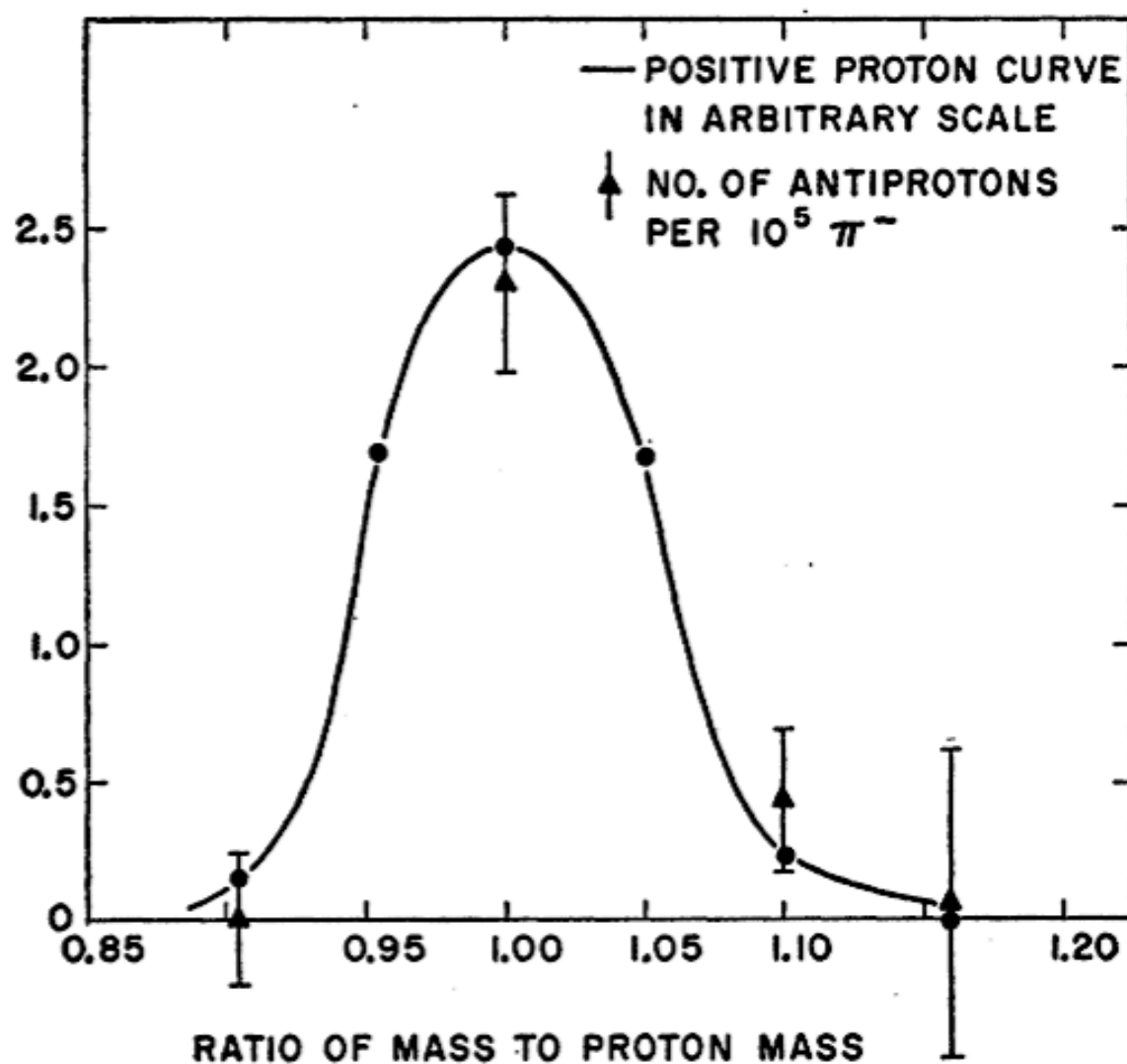


FIG. 4. The solid curve represents the mass resolution of the apparatus as obtained with protons. Also shown are the experimental points obtained with antiprotons.

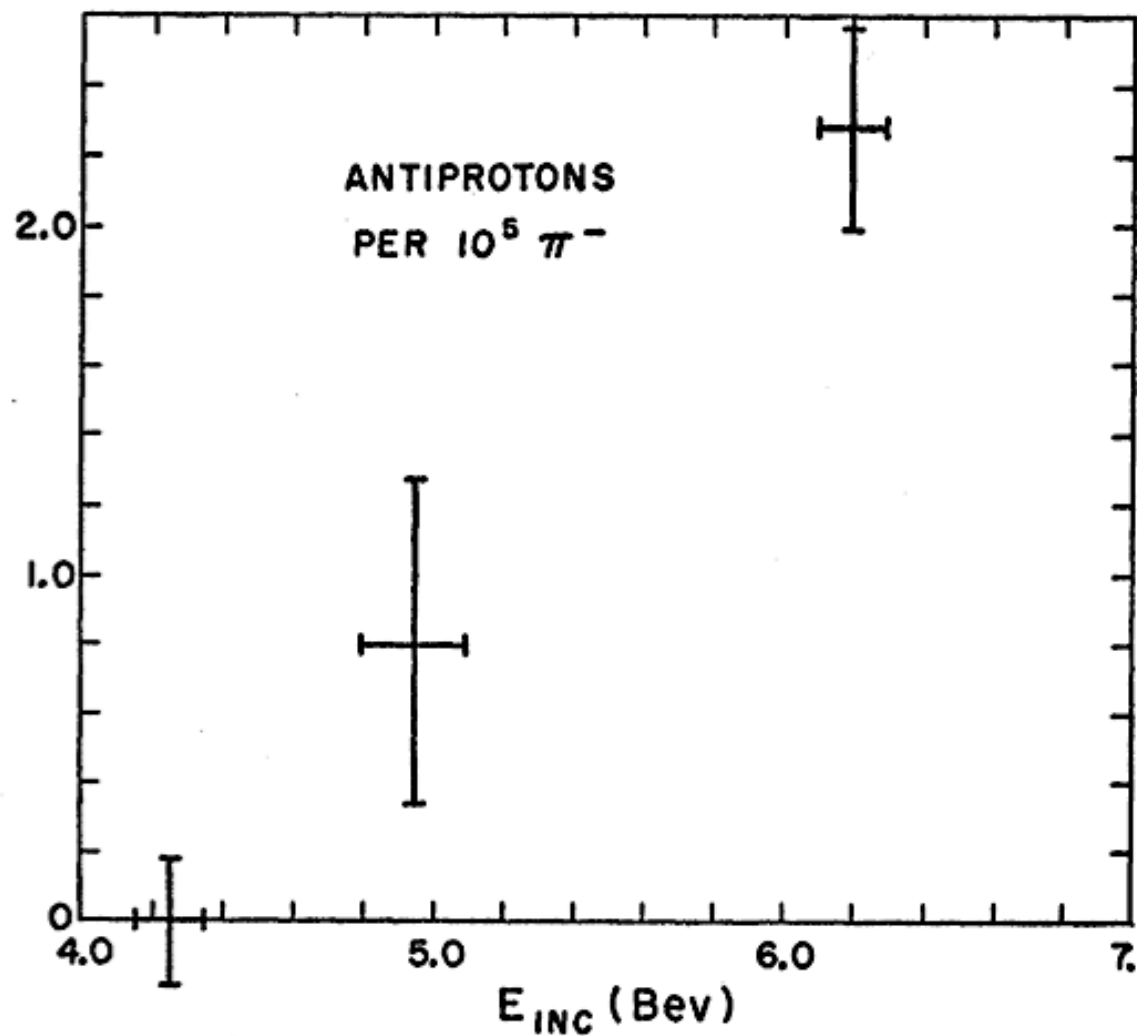


FIG. 5. Excitation curve for the production of antiprotons relative to meson production as a function of Bevatron beam energy.

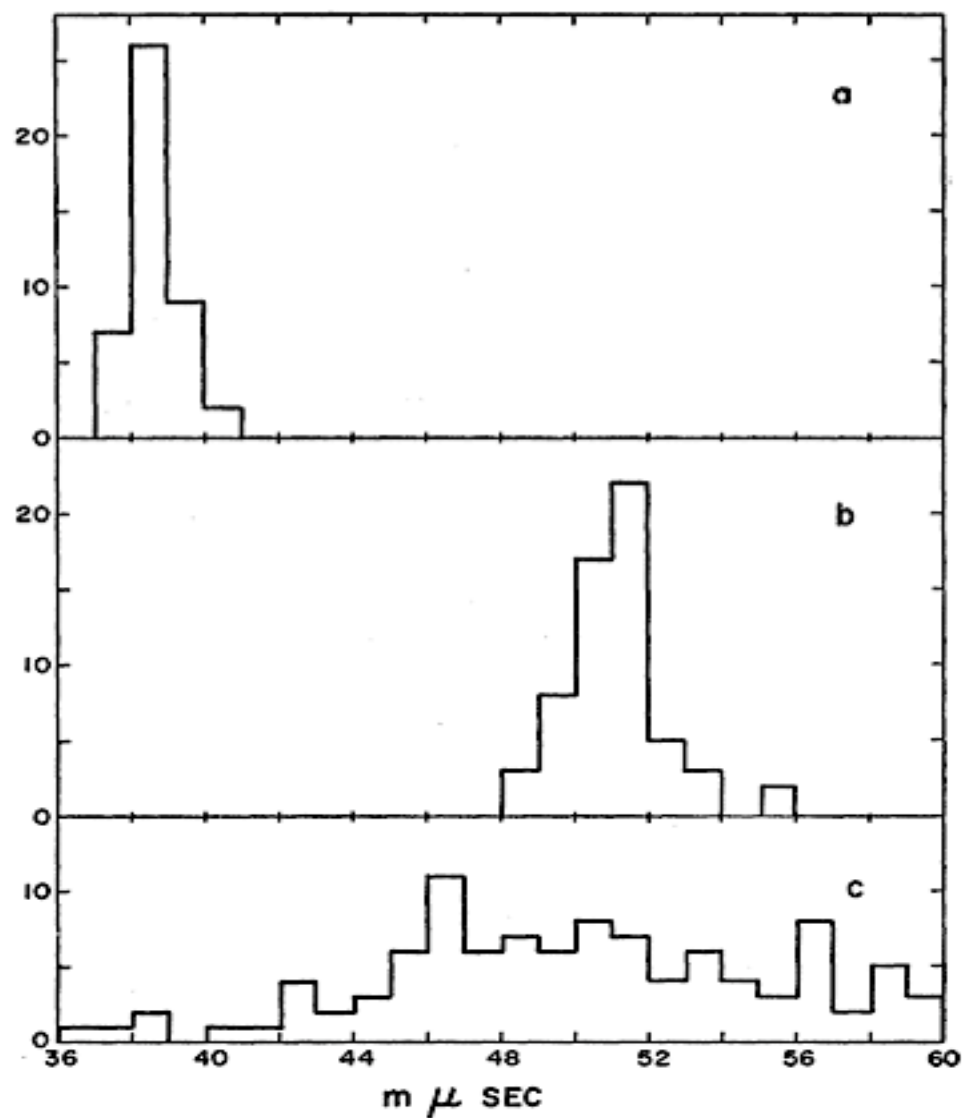
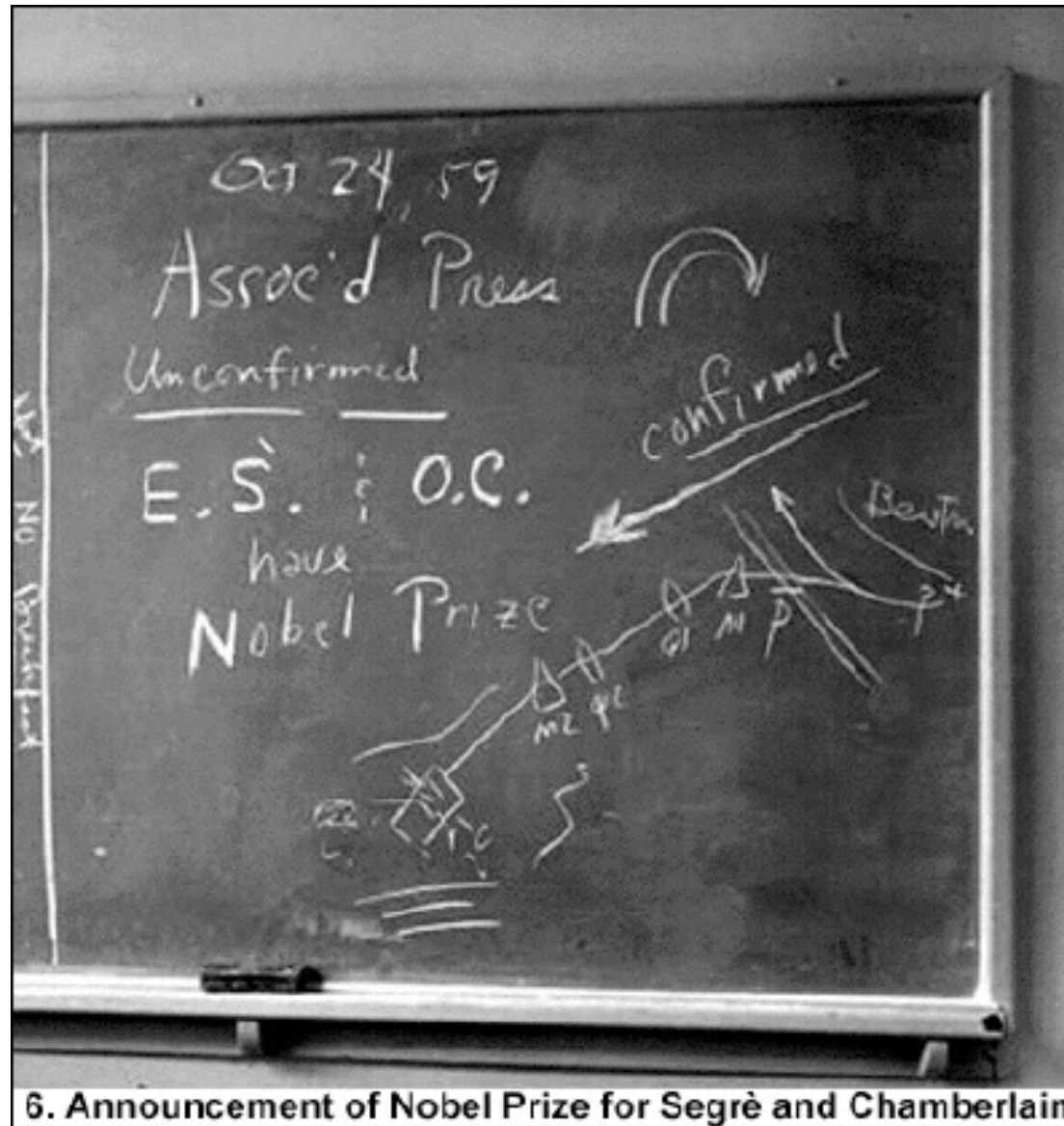


FIG. 3. (a) Histogram of meson flight times used for calibration. (b) Histogram of antiproton flight times. (c) Apparent flight times of a representative group of accidental coincidences. Times of flight are in units of  $10^{-9}$  sec. The ordinates show the number of events in each  $10^{-10}$ -sec intervals.



6. Announcement of Nobel Prize for Segrè and Chamberlain



